

Peer-review report of

Rogbeer, K. G., & Pérez, E. (2026). Solidarity as a bridge: Shared discrimination is indirectly associated with voting intentions among People of Color. *advances.in/psychology*, 1, e328013. <https://doi.org/10.56296/aip00045>

Round 1

Editor Decision

Dear Authors,

Thank you for resubmitting your manuscript, “From Shared Struggles to Shared Votes: The Political Mobilization of People of Color,” for consideration in our *Special Issue on the Psychology of Pushback*. We appreciate the thoughtful revisions you have already made—the multigroup design and theoretical framing now come through much more clearly, and we believe the paper is very close to meeting the threshold for external review.

Before we can send the manuscript out, however, we ask that you address the points below. We anticipate these changes will be straightforward and will substantially strengthen both the transparency and the impact of your contribution.

1. Report the direct (main) effects of the experiment

- Please report the overall main effect of your experimental manipulation on voting intentions for the full sample and as moderated by ethnic group (e.g., Latino, Black, Asian, etc.).
- A repeated-measures ANOVA that crosses *experimental condition* with *ethnic group* would accomplish this.
 - Report *F*-values, *df*, *p*-values, and effect sizes.
 - Accompany each cell with the group mean and its 95 % confidence interval, either as a table or as a figure (again, reflecting the full condition x group interaction).
- Because our field increasingly values evidence about *absence* of effects, please include a direct test of the null hypothesis that the manipulation does not influence voting intentions (e.g., a Bayes factor, equivalence test, or non-inferiority test). This will allow stronger conclusions and enhance the paper's contribution.

2. Align title, abstract, and narrative with the empirical pattern

Our reading of the data (and your preliminary analyses) suggests that the manipulation's direct effect on voting intentions is likely non-significant, whereas the indirect path through perceived inter-minority solidarity reaches significance. That

pattern is valuable, but the current title, abstract, and main text lean heavily on the indirect effect and may lead readers to infer robust changes in voting behavior. Please:

- Revise the title and abstract so that they accurately reflect both the indirect findings and the absence (or equivocal nature) of a direct effect on voting.
- In the Discussion, make explicit why null direct effects are theoretically meaningful—e.g., they help us understand why optimism about cross-group solidarity does not always translate into electoral change in the contemporary United States.

3. Clarify interpretations that invoke the 2024 election

The manuscript states, for example, “*Our finding that shared discrimination catalyzed voting intentions among people of color may seem contradictory to actual voting patterns in the 2024 election, given that Donald Trump narrowly edged out Kamala Harris in the popular vote for president.*” Presenting the full means and 95 % CIs for each group/condition will allow readers to see exactly how those statements map onto the data and to judge whether the cell means plausibly generalize to real-world turnout patterns. Please integrate those descriptive statistics wherever you interpret the election outcome.

Next steps

We clearly see the potential in this work and believe these revisions need not take much additional time. Once you have addressed the points above, please resubmit the revised manuscript along with a concise response letter detailing how each item was handled. Assuming the revisions are satisfactory, we will proceed to external peer review.

Thank you again for choosing to submit your work to our special issue. We look forward to reading the revised version.

Best,

Jonas R. Kunst & John F. Dovidio

No reviewer comments for this round yet.

Round 2

Editor Decision

Dear Authors,

We appreciate you submitting your manuscript, "Solidarity as a Bridge: How Shared Discrimination Indirectly Shapes Electoral Intentions Among People of Color," for consideration in our special issue on The Psychology of Pushback. We have carefully reviewed your work independently and then received valuable feedback from two experts in this field. Their constructive comments, along with our own editorial assessment, have been instrumental in reaching our decision.

We are pleased to invite you to revise the manuscript, addressing the issues raised in this initial round of review. Although the outcome of the revision processes is not guaranteed, we would not invite this revision unless we recognized considerable promise in your work. Of course, the final determination will depend on how well you are able to address the concerns that have been raised.

The reviewers and the editorial team are in agreement regarding the significant strengths of your manuscript. As Reviewer 1 states, "I want to first commend the researchers for a very well written paper on an important topic. I believe this paper offers important insights into not only political psychology but also to intergroup relations more broadly." Similarly, Reviewer 2 notes, "I enjoyed reading this paper - the literature review was thorough and well-written, and I believe the findings on inter-minority solidarity will be of interest to readers of advances.in/psychology. I appreciated the focus on behavior rather than attitudes (policy support), and particularly the authors' efforts to link the findings to the real-world outcome of the 2024 US presidential election." We share this positive assessment. We found the manuscript to be commendably clear and engaging, the question being pursued to be timely and important, and your use of large, representative samples from diverse groups to be very valuable and illuminating.

Nonetheless, both reviewers identified several limitations that warrant careful attention. The reviewers have offered considerable and thoughtful feedback; while we ask that you respond to each of their points, we will not reiterate them here. Instead, we wish to highlight the overarching issues that we and the reviewers consider most critical for revision.

First, as Reviewer 2 highlighted, it is unclear what became of the measure for "intentions to vote for a candidate committed to advancing the political concerns." We ask that you present a model using this continuous outcome variable that corresponds to the model with the binary voting outcome. This analysis is crucial, as it provides a direct test of your hypothesis that the Harris strategy was less appealing to people of color. An effect on this variable would substantially strengthen your reasoning, if absent it needs to be revised.

In addition, both reviewers questioned the suitability of the control condition. Please add a dedicated limitations section to your Discussion that addresses this issue, as well as the constraints of relying on self-report data. To avoid any misunderstanding, please also clarify throughout the manuscript that your focus is on self-reported intentions, not observed behavior. To contextualize this, please cite meta-analytic evidence on the *average* intention-behavior link generally and in voting. Note that the

assertion that this link is often $r > 0.7$ may give a false impression; please present a more nuanced figure that reflects the often weaker associations found in meta-analyses.

Furthermore, we ask that you communicate more clearly that the manipulation included a component emphasizing that the experiences of other groups are similar to those of the participants' ingroup. On a logistical note, the supplementary materials file was not in a standard format (we had to change it to word format); please ensure the revised version is a Word or PDF file on OSF, and clarify the slight variations in the bar charts between conditions.

We appreciate your effort to avoid unwarranted causal language; however, this point requires more careful and consistent attention. For instance, you write that “participants who read about hate crimes affecting their own racial group as well as two other minoritized groups expressed greater PoC solidarity, which then *increased* their intent to vote for Kamala Harris over Donald Trump or another candidate.” The pathway between solidarity and voting intention in your model is correlational and must be described as such. Consequently, any indirect effects should be framed as indirect associations, avoiding causal terminology (e.g., “indirectly shaped”, ll. 334-335). This issue is also present in the discussion, where sentences need careful revision. Please amend statements such as, “Using this design, we found that Black, Latino, and Asian Americans who were primed with shared discrimination reported greater intent to vote for Kamala Harris over Donald Trump or another candidate—a key precursor of political behavior— but only through an increased sense of solidarity with PoC.” Similar phrases requiring revision include, “Our finding that shared discrimination *catalyzed* voting intentions through PoC solidarity...” and “PoC solidarity *impact* vote intentions”. Please conduct a thorough review of the manuscript to ensure all such instances are corrected.

In addition to these points and the remaining issues highlighted by the Reviewers, please address the following:

INTRODUCTION

As Reviewer 1 observed, “the introduction feeling a bit scarce.” We ask that you expand your coverage of work on stigma-based solidarity to strengthen the empirical and theoretical foundation for your research. In addition to the work identified by Reviewer 1 (Sanchez, Chaney, & Remedios, 2018), Maureen Craig has published several important papers on this topic. You cite some of Craig’s (and Richeson’s work) but, given this sustained program of research, discussing the ideas and related results more fully could help bolster the rationale you are developing. Also, you state that a meta-analysis shows that “shared discrimination consistently increases solidarity between people of color,” but a citation is missing. Please provide the reference, as this is crucial for evaluating whether the meta-analysis reports experimental effects that would justify the causal language implied by the word “increases.” In the discussion, you describe this effect as less consistent, which seems to contradict this statement.

Additionally, while the detailed focus on the social position of Asian Americans is valuable, a dedicated heading for this group is not warranted given that there are no parallel sections for the other groups. To maintain structural consistency, please integrate this discussion into a more general section of the manuscript.

METHOD

In your power analysis, please specify the type of design to which it applies. As you are essentially testing an interaction with six distinct groups, greater specificity is required.

You describe your samples as quota-representative. As this is a key strength of your work, please highlight this in the abstract.

Please clarify which software was used to calculate the sensitivity parameters and ANOVAs, specifying whether it was MPLUS or another program.

Finally, please describe the attention check used in your study. You should explain the procedure for participants who failed this check and report the total number of participants to whom this applied.

RESULTS

Please present the main effects from the 2x3 ANOVA before the SEM. These ANOVA results should also include the analyses of the mediator and the continuous voting intentions variable using the same 2x3 framework.

When first using the abbreviation "IE," please define it and specify whether the indirect effects are standardized (strongly preferable). Also, please remove the equals sign following "CI" when reporting confidence intervals.

As the reviewers highlighted, please revise your description of a non-significant trend as a "comparatively weaker mobilizing effect." According to conventional significance thresholds ($p < .05$), there is insufficient evidence to claim an effect. We understand that this cutoff is somewhat arbitrary, but please describe this finding in a more nuanced way that accurately reflects the statistical results.

Given that you find limited differences on the path to the mediator but clearer differences on the path from the mediator to the dependent variable, we suggest that you test a partially constrained model in which only the first path (IV to M) is constrained.

When referring to the meta-analysis on line 417, you are missing a citation. Furthermore, this content belongs in the discussion, not the results section.

Since you did not find differences between groups in the ANOVA, the SEM effect among Latino participants seems to reflect a suppressor effect resulting from the inclusion of the mediator. Please devote more attention to interpreting it as such. The fact that you are partialling out the solidarity effect should help explain why the direct effect becomes negative.

DISCUSSION

This connects to our previous point regarding causal language. We find the following claim to be insufficiently supported by the data presented: “But in doing so, she also forfeited the opportunity to position herself clearly as the prototypical representative of voters of color, which our results indicate would have garnered her wide and steadfast support.” As we noted earlier, a conclusion of this nature would need to be substantiated by an analysis of the continuous voting intentions variable that you have not yet reported.

In addition, it is important that you discuss more fully a finding related to a comment by Reviewer 2. Reviewer 2 wrote, “the authors could offer some explanation for the negative direct effect of perceived discrimination on voting intentions for Latino Americans. This seems a key finding that is contrary to expectations and which I am struggling to understand.” Besides its relevance to your theoretically-based expectations, the pivotal role that the Latino-American vote had in the presidential election outcome makes this result an important one to understand practically, as well. There is currently some consideration of this issue (II. 473-482); we are asking for expanded consideration of these particular findings, including their implications and discussion of the concrete directions future research can take to illuminate the dynamics more fully.

STYLISTIC ISSUES

Although the manuscript is generally well-written, a careful proofreading is required to correct several typos and missing words. In addition, please ensure that the manuscript strictly adheres to APA 7th edition style guidelines. We have noted several areas that require attention:

1. Scale endpoints should be presented in parentheses and italicized (e.g., 1 (*Not at all*) to 7 (*Very much*)).
2. The citations and reference list contain numerous inconsistencies with APA 7 style. For instance, there are missing commas after “et al.”, and “and” is sometimes used instead of “&” for parenthetical citations of two authors. Please also ensure that DOIs are included for all articles and formatted as complete URLs. Our automated citation check revealed several additional issues that must be carefully addressed. The full report can be accessed here: https://drive.google.com/file/d/1OEAZUHu_tvC6gh9LcyU5a1xF16CEP3v8/view?usp=sharing
3. Please always report exact three-decimal p -values, except for when $p < .001$. Be sure to interpret findings just above the $p = .050$ cutoff with greater care. As you know, this threshold is neither fixed nor universally accepted. It would strengthen your work to articulate your perspective, citing relevant methodological papers, and to formulate cautious interpretations of any observed trends (or what a “trend” even represents in your interpretations).
4. It is unclear what “mega-group” stands for.
5. In line with APA7, do not italicize Greek letters, only Latin statistical letters.

6. “marginally reliable” is an odd term. Please revise.
7. In Figure 1, you describe the effects as unstandardized but use the Greek letter beta (β), which typically denotes standardized effects. To avoid confusion, please either use *B* for unstandardized coefficients or, strongly preferably, report standardized effects to facilitate the interpretation of effect sizes. Additionally, 95% confidence intervals and exact *p-values* must be presented for all effects reported in the text and in the figures.
8. The tables are not formatted according to APA 7 guidelines.
9. Table 4 must report both the between-groups and within-groups degrees of freedom.

In summary, we believe this manuscript holds considerable promise, though its potential has not been fully realized in its current form. We therefore invite you to submit a revised version that addresses the points outlined above. Acknowledging that these revisions will require additional space, we have extended the word limit for your manuscript by 2,000 words. Please submit your revision by October 15, 2025. Should you require more time, do not hesitate to contact us.

We look forward to receiving your revised manuscript.

Best,

Jonas R. Kunst & John F. Dovidio

Reviewer 1

Thank you for the opportunity to read and review the paper, which examined how the perception of shared experiences of discrimination can lead to solidarity among different communities of color in the US, and, in turn, influence voting intentions. I enjoyed reading this paper - the literature review was thorough and well-written, and I believe the findings on inter-minority solidarity will be of interest to readers of *advances.in/psychology*. I appreciated the focus on behavior rather than attitudes (policy support), and particularly the authors' efforts to link the findings to the real-world outcome of the 2024 US presidential election. The authors also provided data and syntax to replicate their analyses and I have done so successfully. However, I have a few concerns, mostly requests for further discussion and clarification of a few points:

- The authors could offer some explanation for the negative direct effect of perceived discrimination on voting intentions for Latino Americans. This seems a key finding that is contrary to expectations and which I am struggling to understand. In fact, although there were positive indirect effects via solidarity, there is little evidence overall that the shared discrimination manipulation increased voting intentions for Harris. I think this is an interesting

finding nevertheless (that shared discrimination appeals only seem to help via solidarity) but the positive effects of the manipulation should not be overstated when drawing conclusions from the study.

- It is mentioned that there was a manipulation check, but the results are not reported. Please include in the Results section. If the manipulation was not successful, this would be important to discuss.
- I also could not find the manipulation check question/s - if possible, I would appreciate having access to the full questionnaire on OSF, not just the article (manipulation) text.
- There is a second outcome measure mentioned in the Methods section which is a more general measure of voting intentions for candidates that represent PoC, not specific to any candidate. This doesn't appear to be used in any of the analyses, though this may be a misunderstanding on my part? If I'm mistaken here, the labeling of each outcome in the Results needs to be clearer. If not, this additional measure should either be removed from the Methods to reduce confusion, or included in the Results. I wonder if the effects on this outcome would differ from voting intentions for Harris, which would also align with the researchers' arguments that Harris did not present herself as a representative of PoC's shared experiences.
- I would strongly encourage the authors to pre-register future studies. The omission of the other measure of voting intentions would be less of a concern if I could see what analyses the authors had originally planned.
- It is a limitation of the study that the control group read about a completely unrelated topic. Possibly any effects of the manipulation could be attributed to priming participants to think about race and their own racial group's marginalization. This should at least be acknowledged in the paper, and any future replication could have a control condition where participants read a similar vignette about experiences of racism that only discusses their own group (i.e., only 'Black Americans' instead of 'people of color' and instead of 'Asian and Latino Americans').
- The description of the study procedure and manipulation was a little unclear (bottom of p. 12, top of p. 13) - it implied that participants only read about the two other racial groups but not their own. Perhaps add a sentence to clarify that the text also explicitly mentions that these experiences are similar to their own group.
- I would like to see the exact p values (to at least 2, preferably 3 decimal points) for all effects.
- I spotted a couple of typos - on p. 2 line 26, social inferior should be socially inferior, and on p. 14 line 314, 'paths were' is repeated.

Thank you again for the opportunity to read this engaging and timely paper. I believe this research is publishable pending some clarification on points above.

Reviewer 2

Thank you for the opportunity to review “Solidarity as a bridge...” I want to first commend the researchers for a very well written paper on an important topic. I believe this paper offers important insights into not only political psychology but also to intergroup relations more broadly. My comments below can largely be addressed with revisions and are organized based on their order in the manuscript.

The introduction largely focuses on solidarity research stemming from Pérez and colleagues. While certainly a leading expert on solidarity, especially from a political science perspective, it left the introduction feeling a bit scarce. I would encourage the researchers to more fully integrate other research on stigma-based solidarity and interpersonal allyship. For example, the extent to which people perceive prejudices as stemming from a common source can facilitate Black-Asian allyship (Sanchez, Chaney, & Remedios, 2018). Similarly, I would have liked to see the discussion making shared discrimination salient more thoroughly discussed. Research on linked fate seemed relevant here, as does research on intraminority discrimination versus discrimination enacted by White people. I note intraminority discrimination given the focus on the affirmative action ruling, which was often framed as Asian Americans engaging in discrimination against Black Americans.

It would also be helpful for the researchers to define political solidarity earlier in the manuscript introduction, including considering how political solidarity is typically measured and constructed in the reviewed papers (e.g., voting intentions, policy support, intergroup attitudes). Similarly, it could be beneficial to indicate more clearly how people are typically informed about shared discrimination. As the manipulation articles focus on hate crimes more than interpersonal acts and everyday experiences, it should be noted when this diverges from other relevant research (e.g., Cortland et al 2017 focuses on everyday acts and structural policies; Craig’s word often focuses more on historical or everyday acts of discrimination).

Method

Please present gender breakdown more completely. Presenting only the percentage of women implies a gender binary in its suggestion that the remainder of the sample must be men. Given the large-scale attack on trans folks during the election cycle, it would also be helpful to note how gender identity was assessed (specifically if it included a question asking about cis versus trans identity or offered a nonbinary identity option).

I was a bit confused by the articles participants read for the treatment condition and was unfortunately unable to open the supplementary materials on OSF (this may be user error but I generally do not struggle with OSF files so I encourage the researchers to check on this). Mainly, it would be helpful to indicate more clearly in

the manuscript how assignment to articles worked. Among the Asian sample, did 50% read the Asian and Latino article and 50% read the Black and Asian article? Or did 33% of the Asian sample read each article? Or is it that all Asian participants read about Black and Latino discrimination? Each of these raises various nuances that would shape my reading and understanding of the results, so I was disappointed to not have greater clarity here or to be able to access the articles. Whichever the method, this needs to be stated more clearly and I would encourage the researchers to justify that choice more clearly.

I also wondered why turtles were chosen as the topic of the control article. Certainly, this is race-absent, but it is also U.S.-absent, politics-absent, and far less self-relevant than the manipulation article. What is lost by having a poorly matched control article?

What was the manipulation check question that participants completed? Did everyone have to get it right to progress? If not, what percentage failed this question?

Was a question included assessing if people were registered or eligible to vote? I ask primarily given the much higher percentage of foreign-born Asian participants in the sample and no mention of citizenship. Relatedly, were participants able to indicate if they did not intend to vote? Given poll data suggested that democrats' voting rates were down in 2024 from other elections, it would seem a number of democrats abstained from voting.

Scale reliability scores should be provided for the measure of solidarity with POC. Can the researchers also clarify that the third item of this scale was adjusted to participants' own racial identity?

It would be helpful to indicate descriptive statistics, particularly for voting intentions. Was voting intentions particularly skewed? *I'm returning to this comment to note that the voting intentions means are presented in Table 5 though I would encourage earlier reporting of these and other descriptive statistics.

On page 17 the authors note, "Although the estimate for Asian Americans was only marginally significant, the contrast between groups was statistically meaningful ($\Delta\beta = .16$, $SE = .08$, $p = .05$), supporting our hypothesis." While the cutoff of $< .05$ is of course arbitrary, it is important to note that $p \text{ EQUALS } .05$ is technically not defined as statistically significant. This is repeated in the following sentence for Black and Latino Americans as well.

I was surprised that demographic variables (age, gender, education, foreign-born) were not included as covariates in the SEM model to test for robustness.

Round 3

Editor Decision

Dear Authors,

Thank you for submitting your manuscript, "Solidarity as a Bridge: Shared Discrimination is Indirectly Associated with Voting Intentions Among People of Color," for further consideration in our special issue on *The Psychology of Pushback*.

We have carefully reviewed your revised work, as have the same two experts who evaluated the original version. We appreciate your diligence in addressing each of our concerns. We are all in clear agreement that your paper has improved significantly on all fronts and is close to becoming a strong contribution to the field.

Before reaching this goal, we request that you respond to the Reviewers' last minor comments and address the additional points listed below.

Analysis

- **Attention Checks:** Like the Reviewers, we would like to know whether your findings hold when excluding participants who failed the reading attention check. Usually, the effect should get stronger after such exclusion, though power is naturally reduced. Given the relatively strong effects you observed on the mediator, we assume the general finding will hold. However, even if some of the effects do not hold, we would like you, for transparency, to summarize the results with this reduced sample in the main text and present details in the SOM. If the results with reduced sample do not fully hold, please call readers' attention to this briefly in the Discussion section and speculate why. For example, if the effect sizes are comparable but significance changes, you can suggest it is a statistical power issue. This new element of the Discussion can be brief, but we feel other readers may also wonder about the results. It would thus be valuable to acknowledge differences across the analyses in explicit and productive ways. If the results with the reduced sample produces the same results as your current, main analyses, then no additional discussion is need and a brief summarize in the Results will suffice.
- **Reporting Statistics:** As you report *F*-tests in one of the tables, please do not repeat the specific statistics in the text; instead, simply refer readers to the table earlier in the section.
- **Formatting Greek Letters:** Please do not italicize Greek letters (e.g., eta-squared, α). The use of italics in the tables must follow APA 7 standards, just as it does in the text.

- SEM Model Reporting: Please provide the exact fit indices for your SEM model in the main text, rather than just noting the cutoffs they fell below. In addition to *RMSEA*, *CFI*, and *SRMR*, please report the chi-squared value, *df*, and the *p*-value.
- SEM Figure Details: In your SEM figure, please present precise three-digit *p*-values rather than using asterisks (*). Additionally, please include the 95% CIs in parentheses after each effect (e.g., $\beta = .21$ [.19, .23], $p = .002$). Please explain in the figure note that the parenthetical values reflect 95% confidence intervals.

Minor Stylistic Issues

- Abstract Readability: Please split this long sentence in the abstract into two: “However, Harris’s avoidance of identity-based appeals meant she was not perceived as a clear PoC representative, particularly among Black voters, for whom the indirect association between shared discrimination and voting intentions was stronger when predicting support for a candidate seen as advancing PoC interests than for Harris.”
- Clarifying Meta-Analytic Evidence: Please change the word “consistently” to “on average” in this sentence: “Meta-analytic evidence (Pérez, Vicuña, & Ramos, 2024) indicates that shared discrimination consistently increases solidarity between people of color ($d = 0.18$) and that solidarity, in turn, predicts support for outgroup-advantaging policies ($d = 0.79$).” The reason for this request is that the meta-analysis reports no standard estimates of heterogeneity (e.g., I^2 or Prediction Intervals). The range of effects they observe—and that you report yourself—points to inconsistent effects for policy support, even though an average significant effect is observed.
- Missing word: There appears to be a missing word in this sentence: “Although our study did produce significant direct effects [of] shared discrimination on solidarity, the magnitude of this effect should be interpreted with caution.”
- Italicization: Please italicize *N*.
- OSF Links: For the next, and likely final, version, please provide non-anonymous OSF links and ensure the repositories are public.
- Citations: There are still some APA 7 inconsistencies. For instance, work with three or more authors is usually cited as [First Author Name] et al. (Year) from the first citation onwards.

Final Checks

Finally, please upload your manuscript to our new tool (<https://apa.advances.in>) to check for the coherence of citations and references. As you will see, despite some false positives, there are various references for which the number of authors or years do not match. Please make these adjustments before resubmitting to facilitate the production process.

Given the minor nature of these changes, we request the updated manuscript within two weeks, i.e., by December 5. Should you require more time, do not hesitate to contact us.

We look forward to receiving your revised manuscript.

Best,

Jonas R. Kunst & John F. Dovidio

Reviewer 1

Thank you to the authors for their careful attention to the feedback, I am satisfied that my comments have been addressed. The only additional comment I would make is that the manipulation check seems more like a comprehension check to me - it's testing whether participants understood the information presented more so than it is testing whether perceptions of shared discrimination were successfully manipulated. It's also not clear if this item only refers to participants' own racial group or the two others.

Reviewer 2

Thank you for the opportunity to review the revised manuscript, "Solidarity as a Bridge: Shared Discrimination is Indirectly Associated with Voting Intentions among People of Color." I found the researchers to be highly responsive to previous comments and the present manuscript to be much improved.

In particular, I found the introduction much more compelling and comprehensive with the current revisions, the description of the manipulation article was significantly clearer in the manuscript, I was now able to access the OSF files to view the articles, and the discussion of limitations was successful.

My very minor lingering comment is just that, while I appreciated the additional information about the manipulation check, I do wonder about the decision to retain participants who did not pass the manipulation check (130 + Latino ps, 90 Asian ps, and 119 Black ps). A statement indicating this was for the sake of statistical power would suffice, or perhaps supplemented analyses with a more restrictive sample.

Otherwise, I believe this manuscript makes a strong contribution to the literature. I look forward to citing this paper in my future work.